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*Kreosote in Hæmatemesis—Report of a Case, by*  
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THE powerful styptic properties of kreosote, have been so repeatedly tested by recent observations, that it has obtained, by common consent, a prominent place amongst this class of remedial agents. In hæmorrhages occurring in individuals of enfeebled constitution, and arising from an atonic condition of the vessels of a part, its powers, both as a stimulant and a styptic, would seem to indicate it as a peculiarly effective remedy. This form of disease is presented to us in the hæmorrhage from the stomach, which sometimes occurs in persons addicted to the use of ardent spirits, and it is to such a case that I would direct attention by the following communication.

*Seventh month 17th, 1839.*—I was called about 6 P. M., to see J. C., a tavern-keeper, of about forty-five years of age, a remarkably stout, athletic man, accustomed to the free use of spirituous liquors, and addicted to the pleasures of the table. I had attended J. C. on a former occasion, about two years since, with an attack of hæmatemesis, which was of a moderate character, and was speedily controlled by remedies. I then informed him of the danger of a recurrence of the disease, if he persisted in his dissipated habits,—and my caution produced, for a time, a salutary change. He had, however, returned to his old ways, and I was now called to visit him for the same disease in a more violent form. The attack had commenced in the early part of the afternoon, and he had twice vomited a considerable quantity of blood, mixed with the contents of the stomach. I found that on the preceding day he had been indulging more freely than usual in his potations, and that he had been complaining for some days previously of a numbness and sense of fulness in the back of his neck and head, with loss of appetite, and uneasiness about the stomach. I directed him to go to bed, and keep perfectly quiet, take iced lemonade for drink, and have eight cups applied to the epigastrium, with the following mixture:—*R. Acid Nitrosi gtt. viij., Aquâ Camphor. ʒiv.* To take a tablespoonful every hour, diluted.

I was summoned in haste to see my patient about midnight, and found him pale, and exhausted, with a cool skin, and feeble pulse. He had just had a copious discharge of pure blood from the stomach—I should suppose, about a pint. He complained of a sense of weight at the epigastrium, preceding the discharge, which was relieved by the vomiting. He was incessantly calling for drink. I directed ice to be held in the mouth, and discouraged the use of large draughts of water, to suspend the acid

drink, and take a pill every hour of *Acet. Plumbi gr. ij., Pulv. Gum Kino gr. j., Pulv. Opii gr. ʒ.*

*18th.*—I was called to him at 7 o'clock, A. M. He had vomited small quantities of blood several times during the night, and had had a copious discharge just before I arrived, besides two stools containing dark blood. His pulse was now irregular and feeble; skin cold, pallid, and covered with a clammy sweat. He had slept very little during the night. The pills appeared to sicken him, and he had taken but two. At my request, a consultation was called, and Dr. Otto visited him with me. The pills were discontinued, and we placed him on the muriated tincture of iron, *gtt. x.* every hour. Ice and cold drinks continued, and an anodyne enema; sinapisms to the extremities. We met again at noon, and found our patient still extremely feeble; he had vomited several times a small quantity of blood. We concluded, if it should return in large quantities, to try the kreosote. I saw him again at 4 o'clock. He had just vomited a large quantity—I should suppose, a pint—of dark blood, containing coagula. His pulse was extremely feeble, and skin as cold as marble. He had settled his affairs, and looked forward to death as inevitable. I directed a large blister to be placed over the abdomen, sinapisms to the extremities to be renewed, and take a dessert-spoonful of the following mixture every hour:—*Kreosote gtt. viij., Pulv. Gum Arab., Pulv. Sacch. Alb. aa ʒj., Aquâ ʒij.*

*9 P. M.*—Had no return of the bloody vomiting since taking the kreosote. He had vomited once after the second dose, but merely threw up the fluids from the stomach, without any admixture of blood; the odour of the kreosote was very perceptible in the fluids discharged. Slight reaction had occurred, with an evident tendency to delirium. The kreosote was directed to be continued through the night every two hours, with black drop *gtt. x.* every two hours.

*19th.*—Pulse improved; skin warm; blister has drawn freely; patient dozed at intervals during the night; talked wildly on waking, but was rational when roused; desires food; no return of vomiting. He was allowed nutritious drinks in small quantities, frequently repeated, and his medicines were given at longer intervals. He continued to improve from this time, and at the end of ten days was able to move about his house.

In this case, the kreosote certainly produced a prompt and powerful impression, and apparently rescued the patient from death, inasmuch as the remedies generally employed in such cases failed to produce any effect.



## FOREIGN CORRESPONDENCE.

## LETTER FROM DR. BEAUGRAND.

PARIS, June 9th, 1839.

The medical community of Paris has just witnessed one of the most remarkable scientific discussions which has for a long time taken place, before the Royal Academy of Medicine.

The advantages of the experimental method, *exclusively* employed in physiology, the doctrine of Charles Bell, relative to the division of nerves into sensitive and motor, have just been questioned and powerfully shaken by M. Gerdy, notwithstanding M. Blandin's able defence of them. Now that the discussion has terminated, let us give a retrospective glance, and observe what results we may arrive at.

We stated that there were two litigated points.

1. The precise definition of the sense to be accorded to the phrase, *experimental method*, and the physiological value to be attached to it.
2. The specifications of the functions of the nervous system. Let us examine, successively, these two questions.

What is to be understood by the term *experimental method*? Physiologists have, as yet, come to no agreement on this subject. Some, with MM. Bouillaud and Rochaux, wish to express by it the sum total of the means necessary to arriving at the discovery of the truth; that is to say, observation, ratiocination, direct experiments, &c. Others, M. Gerdy at their head, contend that it is the art of interrogating nature, that she may be compelled to answer; in a word, the art of experimenting in physics and in chemistry, and of physiological vivisections. Observation, then, with them, consists simply in the application of the senses to the study of phenomena which are spontaneously produced: whilst experience is in the sum of knowledge acquired by divers means. We willingly assent to this latter opinion, and, in fact, the experimental method is and can be nothing more than a method based on experiment, and all agree in considering this as synonymous with vivisection. Let us cease, then, to confound things so essentially different, and let us see what experiments can teach us in physiology.

When a living animal is dissected, it is necessary, in order to examine an organ whose functions are to be studied, to cut or remove the tissues concealing this organ from the eye of the physiologist. He may readily believe, then, that the severity of the injury inflicted must exercise an unfavourable influence over the wished for results.

How do we know that the integrity of the mutilated parts is not requisite for the accomplishment of the normal phenomena?

This applies especially to experiments upon the cerebral and spinal nerves; besides, can we compare that which goes on in the animal, amid such tortures, with what is taking place in man during health or sickness? It is, to say the least, doubtful, when we reflect that the subjects of these experiments are usually selected from the inferior orders of animals. The organization of some, like the batracians, differs greatly from ours. Have we not repeatedly witnessed frogs and other reptiles surviving the most frightful mutilations? Have we not seen geese walking and swimming for some moments after sudden decapitation? and if we descend to the bottom of the zoological scale, among the polypi, actineans and star-fish, do we not find these animals enjoying sensation, not only without the intervention of a brain, but even without any appreciable nervous system. Thus, comparative anatomy and physiology can throw but a feeble light upon human physiology. Here, as M. Gerdy remarks, it becomes a vacillating and treacherous guide.

But if any thing can demonstrate the uncertainty attending vivisections, it is, without doubt, the diversity of results obtained by experiments upon one and the same question. This last remark enables us, without farther delay, to enter upon the important discussion concerning the nervous system; a subject which has been agitated for such a length of time, and which is still involved in obscurity.

First, however, let us rapidly review the opinions of ancient authors upon this matter. The first idea of distinguishing the nerves into sensitive and motor, belongs to the Alexandrian school. The testimony of Galen is precise on this particular, (*De Loc. Affect. lib. iii. cap. ult.*) It had been noticed that in paralysis, sensation and motion might be lost separately or together, and this phenomenon had aroused the attention of observers; they asked themselves by virtue of what anatomical conditions this could take place; but the subject baffled all the talent of Herophilus and Endemius. Galen, in that portion of his work devoted to physiology, (*De Usu Partium, lib. viii.*) establishes, in opposition to the opinion of Aristotle, that the brain is the source of voluntary movement and of sensation, and that two orders of special nerves preside over these two faculties. The nerves of sensation and of the senses are softer, and spring from the



anterior portions of the encephalon, (cerebrum,) while the nerves of motion are harder and less tortuous, and arise at the posterior and most consistent part, (the cerebellum.) It would appear from some obscure phrases, interrupted by numerous digressions, that he believed the nerves of sensation penetrated deeply into the central soft part, whilst the motor nerves emerged directly from the firmer and denser surface. These ideas were reproduced by the Arabs, (Aricenna, lib. i. p. 59,) with the exception of Averrhoes, who maintained the ancient doctrine of Aristotle, and continued to regard the heart as the centre of the faculties, which Galen, with the Alexandrian school, referred to the brain. The doctrine of the physician of Pergamus, found in the sixteenth century an energetic supporter in the anatomist, Desmoulins. The great Fernel has slightly modified the Galenic theory on this point, as on every other. He thinks that the nerves of the senses spring from the anterior part of the cerebrum, and the motor nerves from the posterior, but he adds that those which preside over the sense of touch originate in the membranes. In favour of this notion he alleges that the brain, being insensible, cannot perceive the sensation of touch, while the membranes, being endowed with a lively sensitiveness, have an aptitude for receiving these impressions, (De Anima Facult. lib. v. cap. x.)

The emptiness of these distinctions was afterwards discovered, and Van Swieten, in his Commentaries on Boerhaave, (b. iii. p. 343 and 455, ed. in 4to.) after having proved that sensation and motion must have nerves of different origins, admits that they are entirely unknown.

I will not detail the ideas of the moderns on the distinction of the nervous system of animal functions, and of that of the functions of relation, as it has no connection with the subject under consideration; so that I finally arrive at Charles Bell. This excellent physiologist says, that the nerves may be ranged into three classes; those of the first class, arising from the anterior columns of the spinal marrow, presiding over motion; the second, coming from the posterior columns, transmitting sensation; whilst the third, given off by the lateral tracts, reserves to itself the direction of special movements—those of respiration. The anatomical data on which Charles Bell founded his system, as well as the experiments which seemed to sustain it, are well known; we will not, therefore, repeat that with which every one is acquainted. Magendie, in France, arrived at analogous results by the too exclusive method

of verisection, and was induced to admit as true, first, the distinction between the motor nerves (anterior) and the sensitive, (posterior,) but not so entirely as the English physiologist had advanced; secondly, that the theory of the respiratory nerves was false: this constitutes the first and remarkable difference in their opinions.

On the other hand, Bellingheri employed himself in the same researches, and following the experimental plan, was led to proclaim that there existed an antagonism of motive action in the anterior and posterior columns of the spinal marrow; the first, presiding over extension, the second over flexion. That sensation resided in the cortical substance, and motion in the medullary. Schapp, Herbert, Mayo, M.M. Fodera, Calmeil, Gerdy, and others, have obtained still more discordant results. Such was the state of the question when M. Blandin roused the debate of which we have to give some account, and which has occupied the Academy during nearly two months.

Three orders of facts, anatomical data, experiments and physiology have been invoked, each in its turn, with a view to attack or defend the speciality of action of the nervous system. These we will review in order.

It is said that there is a perfect similitude between the spinal nerves and the fifth pair.

All have two roots; one with a ganglion, *for sensation*; the other without, *for motion*; thus the spinal nerves and the facial regulate these two orders of phenomena.

Let us examine this first proposition; but, before doing so, we should notice that the filamentous origins of the spinal nerves do not spring from the central portion of the spinal marrow, but from the most superficial layer, in such a way, that, according to some anatomists, Desmoulins among others, the apparent union is merely a juxtaposition.

Is the case the same as regards the fifth pair? Assuredly not. Several of the most respectable authors, Gall, Meckel, Cloquet, &c., assign three roots to it; but let us admit that it has actually but two, the one superficial, the other deep seated. The radicles which compose the first, or motive branch, do not come from the anterior portion of the spinal marrow, but, from the *tuber annulare*; those radicles, by the junction of which the ganglionic or sensitive root is formed, dive deeply into the *tuber* and divide into two fasciculi, one of which proceeds transversely to reach the aqueduct of Sylvius, while the other plunges vertically into the substance of the corpus restiforme.



These origins are very variable. Now, if, as every one admits, the nerves owe their properties to the parts where they arise, can it be said that the spinal nerves and the fifth pair are similar? for, if we have recourse to the functions for the establishment of this pretended similitude, how happens it, we ask, that a division of the trigeminus produces on the other nerves of the senses such remarkable effects? paralysing, as it does, the smell, taste, and vision; inducing, too, acute ophthalmia, a loss of sight, whilst a section of the spinal nerves produces merely paralysis of the sensation and touch. It can be shown, moreover, that the olfactory and optic nerves do not come from the posterior part of the spinal marrow; and also, that notwithstanding their simplicity of action, they have a complex origin; finally, that the different movements of the eye have relation not only with the anterior portions of the spinal marrow, but with other and very different parts of the encephalon.

M. Blandin has observed, that, in man, the nerves of the thoracic members have the posterior root more developed, relatively to the anterior, than the nerves of the pelvis, and hence concludes that this disposition has reference to the fact, that the sensibility of the superior members is greater than that of the inferior. We might content ourselves with saying that M. Blandin has reasoned in a vicious circle, having taken for granted that which in truth has to be proved; but it is better to examine the questions themselves.

In the first place, is it a fact that the members are organs of sensation rather than of motion? secondly, do the inferior members possess less sensitiveness than the superior? Authors have greatly exaggerated the sensitiveness of those parts destined for touch; the human hand, for example, being rather an *organ of motion*. This doctrine appears paradoxical, because it is at variance with certain ideas which have been received without due examination; but let us reflect a little, and ask ourselves whether we do not use the hand more as an instrument of prehension than of tact. Is not the hand, in all professions, more frequently employed in seizing, grasping, and turning, than in feeling? and if the hand is better adapted for this last action than any other portion of the body, is it not owing to its form, to the number and proximity of its articulations one with another, which allow of its intimate application to all bodies, and thus enables it to appreciate every variety of form and density?

This conducts us directly to the second question.

It is quite erroneous to suppose that the superior members are more sensitive than the inferior. The pulp of the fingers is not in the least affected by that tickling which is intolerable to the soles of the feet or the sides. Is the sensitiveness of the hand comparable to that of the genital organs or anus? yet the ganglionic roots which are distributed to these last mentioned parts are smaller than those of the thoracic members. The anatomical remark, then, of M. Blandin, far from befriending the partisans of Charles Bell, is, on the contrary, completely hostile to them.

But are the experiments more favourable to them? The rapid exposition which we have made of the results which the labours of Charles Bell, MM. Magendie, Bellingheri, Schapp, Herbert, Mayo, and the contradictory ones of MM. Fodera, Calmeil, and others, have led to, have already answered in the negative, it being impossible to reconcile with each other the facts reported by them. We have elsewhere seen what amount of importance is to be attached to vivisection and comparative anatomy.

Much stress has been laid on the following experiment of Charles Bell, as already demonstrating the distinction, into motor, and sensitive nerves. On irritating the posterior roots of the spinal nerves in an animal recently killed, no result was obtained, whilst the same treatment directed to the anterior roots produced motion. Charles Bell performed this experiment but once. "But supposing the effect to be constant, what does it prove? The animal is *dead*; there is no longer *any communication between the brain and spinal marrow*. Should we hence conclude that the posterior roots are devoted exclusively to sensation and the anterior to motion? Surely not, and Charles Bell has taken care not to make so sweeping a deduction. He closes his narration of this experiment by saying, that the anterior and posterior columns of the spinal marrow have *different functions*. He, then, does not look upon the question as settled by this single fact. (Charles Bell, *Expos. du Système*, Nat. Paris, 1825. p. 17.)

Let us turn now to the pathological facts. Not to speak of the numerous and well authenticated cases of *complete* destruction of the spinal marrow within a limited space, in which the portions of the body *below* the injury *retained* sensation and motion, several instances are reported of considerable softening of the anterior columns, unfollowed by paralysis of motion, and of similar lesion of the posterior column where sensation was unaf-



fect. The work of M. Ollivier (d'Angers) on diseases of the spinal marrow, contains several very curious examples of this kind. What conclusion, then, must we draw? That Charles Bell's system is entirely false? Certainly not. It possesses some truths of the deepest interest, but which must be taken in a less absolute sense than they have been heretofore. These are precisely the truths which have gained this doctrine all its credit. Thus, for instance, the facial is a motor nerve, rather than a nerve of sensation, yet it is not entirely destitute of sensitiveness, for it has repeatedly exhibited symptoms of this where it emerges from the stylo mastoid foramen, and the sensibility of the anastomotic filaments which it receives from the fifth pair, below the irritated spot, cannot be adduced in explanation of this; we may add, too, that several cases of facial neuralgia are on record which cannot be accounted for by the slender communication afforded by the trigeminus. As to the functions of the spinal marrow, nothing has been actually demonstrated; perhaps, at some future period, the functions of the different component parts of this organ may be discovered; so far, however, opinions and facts are equally at variance.

We hear it said, and incessantly repeated, that the system of Charles Bell can alone explain partial paralysis of sensation or motion.

From the most remote antiquity, this phenomenon has excited the attention of medical men, and each one has presented us with his theory. The one which appears to me the least objectionable is that of Mercurialis. One or more of the different faculties of organ, he says, may undergo change independently of the other; now, sensation and the property of communicating motion are two distinct faculties, and can, therefore, be separately destroyed, (Med. Pract. lib. i. cap. xx. p. 113.) All authors, moreover, have remarked, that in the majority of cases motion was the function lost, or was at least soonest affected; it being a very complex phenomenon, and one demanding, more than any other, the integrity of a greater number of parts.

In fine, we have witnessed the theory of Charles Bell at fault, as regards certain facts in which the anatomical lesion did not correspond to the functional disorder.

As the discussion of which we are speaking was about terminating, M. Magendie communicated to the Academy of Sciences the results of some experiments on the nervous system, which I will briefly state.

The spinal nerves of sensation and of motion are equally sensitive when both are uninjured.

If the nerve of sensation be cut, the motor nerve instantly loses its sensitiveness. Should the motor nerve be divided in the middle, the extremity which remains attached to the spinal marrow is entirely incapable of sensation; the opposite end, however, preserves it.

In this case the sensibility proceeds from the circumference to the centre.

If the nerves of sensation are divided in the middle, the end which remains in contact with the spinal marrow is extremely sensitive, and on the contrary, that position nearest the ganglion loses this faculty entirely.

M. Magendie proposes investigating whether this influence of the sensitive on the motor nerve will not be kept up in the spinal marrow, between its component fasciculi, which may themselves be distinguished into sensitive and motor. All then, again, is reduced to the question, what becomes of the previous experiments which were confidently advanced as sustaining the assertion that the posterior roots are insensible? Perhaps, it may be said they were badly conducted; yet, what shall warrant us in giving the superiority to those now in progress? What judgment are we to form in the midst of such a chaos? We must wait, and hesitate in giving credence to opinions in a department of physiology, of which the foundation is scarcely laid.

## BIBLIOGRAPHICAL NOTICES.

*A Short Treatise on Typhus Fever.* By GEORGE LEITH ROUPELL, M. D., Physician to St. Bartholomew's Hospital, &c. London: 1839.  
*Reports from the New York Hospital.* By N. SHOOK, late Resident Physician. (From the *New York Journal of Medicine and Surgery.*)

THIS work is from a physician at St. Bartholomew's Hospital, and contains some useful instruction for the management of typhus fever. At the same time, it proves that the author is but indifferently acquainted with the present state of our knowledge upon this subject.

He states, what every one who has studied the subject is fully aware of, that typhus is an exanthematous disease, closely allied to the fevers which are commonly called eruptive, such as measles and small-pox. But the eruption in typhus is so rarely wanting, that the disease is very often designated as petechial fever, from the peculiar measles-like rash, differing from true petechiæ, but more nearly resembling them than



any other eruption. Dr. Roupell fancied that this was an original discovery, until he chanced to discover that Dr. Hildenbrand, of Vienna, had arrived at the same conclusion. The lateness of the discovery arose from the great rarity of the work of Dr. Hildenbrand in England; not a single copy in the original could be found in any of the public libraries at London, and the author quotes from a French translation. This is the more singular, as an edition of the work in English was published in this country about ten years ago; it was translated by Dr. Gross, of Cincinnati.

The author afterwards expresses his surprise that Dr. Louis should have laid so much stress upon the lesion of the intestinal follicles in typhus, and disagrees with him as to the frequent occurrence of the lesion; although he regards the diseases described by Dr. Louis as identical with the fever which is almost endemic in Great Britain, and frequently assumes an epidemic form. This is clearly wrong; there is no doubt that typhoid fever exists in Great Britain, just as it does in this country, and that it presents precisely the same anatomical characters as the typhoid form of Paris,—but, on the other hand, the true typhus is without these lesions, which occur only in typhoid fever. It is true that during an epidemic of typhus, the cases of typhoid fever which occur, may assume somewhat of the characteristic appearance of the prevailing disease,—but this is equally true of pneumonia, rheumatism, or any other acute disease.

The typhus of London in 1838, was very evidently contagious, or, if we please to call it so, infectious, the disease transmitting itself by the exhalations from the patients; hence a great mortality occurred among the physicians and pupils attached to the hospital, as well as the nurses. When a local epidemic occurred at Philadelphia in 1836 and '37, it was very evidently infectious—but few of the nurses attached to the wards in which there were many cases of fever, escaped.

The treatment of an exanthematous disease is, of course, not specific. Still much may be done to relieve the symptoms, and diminish the mortality. The remarks of Dr. Roupell on this subject, show that he is a better practitioner than pathologist. The treatment, in fact, resolves itself into the following general conclusions:

Certain epidemics, and certain periods of an epidemic of typhus fever, require venesection, or at least it will be found beneficial in the majority

of cases. To decide as to its propriety, however, there will be much tact necessary on the part of the physician. In general, emetics at the commencement, slightly stimulating diaphoretics, sponging with tepid water, or vinegar and water, are most useful in aiding the natural energies of the system to bring the disease to a close. When there is deficient reaction, particularly towards the close of the acute febrile excitement, wine, or some form of alcohol, is often indispensably necessary. The advantages of this remedy in certain stages of typhus, have been long known, and scarcely ever called in question by sound practitioners. In Philadelphia, the great success of Dr. Parrish in the management of typhus, in 1812 and 1813, arose mainly from his early adoption of this treatment.

The mortality in fevers, it will be seen, is very various. Remittent fevers are strictly under the control of art; and although extremely fatal when of the congestive or malignant form, they may usually be cured by judicious management. Typhus and typhoid fever are not entirely under the control of medicine; hence, if they occur as an epidemic, the mortality must often be considerable, for it depends in a very great degree upon the severity of the disease at a particular season. Thus, we have seen the true typhus in one year prove fatal in a third of the cases—in the next year scarcely destroy one patient in thirty; that is, none died, except a few, who, from some accidental cause, were so enfeebled, that a very slight disorder would prove mortal.

From a report published by Dr. Shook in the *New York Journal of Medicine and Surgery*, we extract the following interesting remarks on the treatment of remittent fever. The success obtained at the New York Hospital was certainly very good; inasmuch, as of ninety-four patients admitted during the season, only four died—in two of which only, were pathological examinations made. The spleen was extremely enlarged, and softened, the liver enlarged, stomach inflamed, but other viscera free from important disease.

The cases treated at the New York Hospital, were similar to many that are seen at Philadelphia, from the coast of North Carolina. We found that a supporting treatment, with the very early use of quinine, wine, and diaphoretics, was imperatively necessary. The experience of the French army physicians on the northern coast of Africa, accords with our views as to this prac-



tice. When, in the congestive, prostrate stage of the fever, blood was abstracted from the patient, the effects were decidedly injurious.

Twenty cases of typhus fever were received into the New York Hospital, during the summer of 1836. These were Irish emigrants; the disease occurred at sea. The remarks of Dr. Shook are in exact accordance with those which we published on the typhus, as observed at Philadelphia, in 1836—'7. The disease ran a determined course,—no specific treatment was discerned, and no decided lesions were observed after death. Glands of Peyer perfectly healthy. Of the twenty cases, six died.

Ten cases of typhoid fever were admitted. Four of these cases terminated fatally. The characteristic disease of the glands of Peyer was present, with perforation into the peritoneum in one of the cases. We are much pleased that the researches which we believe we were the first to make as to the pathology of the fever in the United States, have been fully confirmed by the observations of Dr. Shook. The following remarks are made by him relative to typhoid fever. The memoirs to which he refers, were published in the American Journal of the Medical Sciences, in the years 1835 and '37:

"The symptoms and anatomical lesions all resembled each other, but differed very materially from those of remittent and typhus fever. It is the form of fever which is so common at Paris, and which has been studied with extreme accuracy by Louis and Chomel. Dr. Gerhard, of the Philadelphia Hospital, has given a very able report of the same fever, as witnessed by him during the summer of 1835—'6, showing that the disease differs very much from true typhus, or typhus gravior, ship or jail fever; also, showing from a number of cases examined after death, that the disease is identical with that witnessed at Paris, called typhoid fever, or typhoid affection, or from its anatomical characters, dothinen-teritis, (all names belonging to one disease.) The *post mortem* examinations of the four cases that terminated fatally, were carefully noted, as were the symptoms, treatment, and history, previous to death. The anatomical lesions were the same in every case but one, differing, however, very much from those observed in typhus fevers."

The mode of treatment pursued in the cases of remittent fever, is as follows; the extremely small proportion of fatal cases, shows that it was very successful:

"*Remarks on the Treatment of Remittent Fever.*—It appears from the treatment of the preceding cases, that *tonics* were the principal remedies employed. I am aware that tonics have been long given in the stage of convalescence from remittent fever; but the administration of wine,

quinine, serpentaria, and nourishing soup, to a patient with a dry, harsh, cracked tongue, dry, hot skin, pulse one hundred and ten, severe aching pains of the limbs, and delirium without any previous depletion, is a practice not so common. It is said that tonics should not be administered when the tongue is dry, and the skin hot and free from moisture, and when there is great pain in the head, with a rapid pulse—in just such cases were wine, porter, and quinine, given with the most gratifying results. The skin became soft and moist; the pulse more calm; the delirium subsided; the tongue immediately began to show that the mucous membranes were acted on, and that an altered state of the secretions was taking place. Tonics not only produced a gradual and permanent influence on the appetite and strength of the patient, but they produced an immediate impression. The improvement was sometimes so rapid as to be very remarkable from one day to the next.

A doubt may arise as to the propriety of using tonics in the early stage of the fever; they were used in all the stages, although at times disadvantages followed their employment; occasionally the quinine would irritate the stomach and increase the fever; if so, (which was rarely the case,) the medicine was discontinued until the severity of the symptoms abated.

The *quinine* was always given in solution, and in small doses; if the excitement was great, the use of it was suspended in the evening.

Immediately after admission into the hospital, the patient's bowels were evacuated by some mild purgative, either rhubarb and magnesia, or castor oil; calomel was seldom employed. The following day, if there was a distinct remission of the fever and no local inflammation to prohibit it, the quinine was given; in most cases, it was not given before the third or fourth day after admission. It was usually continued until the stage of convalescence; together with the quinine was administered an infusion of the roots of the *aristolochia serpentaria*—an ounce to the pint of boiling water. This was given to the patients as a common drink in all stages of the fever. If too bitter and strong, it was diluted with cold water.

*Porter* was given in many cases with decided benefit; but if it produced diarrhoea, it was at once suspended. *Wine-whey* was given to all the patients that were admitted late in the disease with great prostration and exhaustion, even if the tongue was perfectly dry, and the gums covered with sordes; it was surprising to see the rapid improvement in such cases. When given in conjunction with porter, the quantity was about eight ounces daily; but as a temporary prescription to obviate extreme prostration, it was not restricted to this quantity. If care be taken to refrain from wine when there is acute inflammation of some organ, no inconvenience will result from its use.

The *spirit of Mindererus* was given in every case of fever as a sudorific; how far it had the desired effect, independent of the other remedies,



I cannot say, as it was always given in conjunction with them.

The frequent *ablution of the head* and limbs with cold water, was found very agreeable to the patients, and of a good deal of efficacy in equalizing the capillary circulation. Ice water to the head during the height of the fever, was one of the most useful remedies in abating the febrile excitement.

*Anodynes* were given with the most happy effect; nothing appeared to allay nervous irritability and restlessness so well as an anodyne; either ten grains of Dover's powder, or a few drops of the solution of sulphate of morphine. It quieted the patient, abated the delirium, and induced sleep. From the decided benefit that morphine produced in most cases, I should class it amongst the most useful remedies in remittent fever."

*Treatise on the Eye, &c.* By WILLIAM CLAY WALLACE, Oculist. Second Edition. New York: 1839. 18mo., pp. 88.

A little volume intended for popular use. It contains a large number of very well executed wood cuts, and is a very lucid exposition of the physiology and comparative anatomy of the eye, as well as of its chief diseases. We think, however, that the author has recommended too highly strychnine, and other dangerous remedies, in the treatment of slight diseases. These are always attended with risk, even under the care of a professional man.

## CLINICAL LECTURE.

### ON TUBERCULOUS DISEASE.

#### LECTURE II.

#### *On the progress and termination of Tubercles.*

By W. W. GERHARD, M. D.

At my last lecture I described to you the influence which inflammation frequently exerted in producing the secretion of the tuberculous matter; but I stated, at the same time, that other causes, of a much more general nature, were also actively engaged in preparing the way for this morbid product, and that in some cases these general causes were sufficiently powerful to bring about a secretion of tubercle from the capillary vessels, without the slightest appearance of a previous local disease. Anatomically speaking, the disease only exists when tubercle is actually formed, and is visible to the eye; pathologically speaking, the tuberculous disease exists before the most minute particle of this substance can be discovered. This apparent incongruity, of course, depends merely upon the obscurity of our language, and the impossibility of detecting, in many cases, the changes which precede the formation of tubercle. If it were the custom to call inflammation the purulent disease, it is very clear that although a diseased action does un-

doubtedly exist previously to the formation of pus, the term purulent disease would seem to be a contradiction when applied in this general manner. But, as the changes which take place in the anatomical structure of the body previously to the actual deposition of tubercle, to a great degree, escape our investigation, we are altogether obliged to lay aside the study of tubercle, which constitutes the anatomical character of the disease, until it is distinctly elaborated; but we do not, on that account, separate the early symptoms of the disease, and the constitutional changes which accompany them, from those which occur in its subsequent stages.

The tuberculous matter is secreted under several different forms. The most frequent is the rounded granular tubercle. This is found in many different tissues of the body, and is obviously not formed exclusively in those organs which contain cavities lined by a mucous membrane. These granulations are discovered in the spleen, serous membranes, mucous follicles of the intestines, and especially in the lungs. In all these parts, the form of the granulations is the same, and in all they present, at their commencement, the same whitish, semi-opaque colour. The appearance of the granulations would indicate that these are formed by a liquid which has been suddenly consolidated, as soon as the globule had attained the size of the head of a very small pin. When the granulations are attached to the serous membranes, they are evidently quite solid, but in the mucous, thin structure, is not so evident in all cases. When they have reached a certain size, their centre sometimes is less solid or completely excavated, so that the walls of the tubercles remain after its central portion has been hollow; this is perfectly shown when the granulations are found in the acini of the liver. It is very clear that the opinion of Dr. Carswell, who states that the granulations are only secreted by the exhalent surface of mucous membranes, is entirely inadmissible; for the serous tissues, and, above all, the parenchyma of the spleen, afford the best examples of the granular tubercle. The lungs present two kinds of granulations; in one, the colour is opaque from the beginning, and the form is somewhat irregular. These are connected in many cases with the bronchial tubes, by little peduncles, or attachments of tuberculous matter, which run into them, and are obviously formed upon the surface of the mucous membranes, and are a direct secretion from the bronchial tubes and vesicles. These tubercles are more opaque and more yellow than the proper gray granulations, which only become opaque after a time, and present a remarkably stellated appearance, from the numerous radicles or processes sent from them into the bronchial tubes. There are, therefore, two distinct varieties of granulations observed in the lungs, while in the other organs of the body this division is less apparent. When tubercles occur in the lymphatic glands, they may be detached at an early stage as minute yellowish points, in the midst of the



proper structure of the gland. These points gradually became more and more numerous until the original structure of the gland is completely replaced by the tuberculous deposit. The proper tissue seems to have been removed by a process of absorption, which goes on, step by step, with the increase of the tuberculous deposit.

There is another form of tubercle, known under the name of tuberculous infiltration. I usually compare this variety, in my lectures, to the melted wax, which is introduced throughout the meshes of a sponge in making a tent. In its early stage the infiltrated matter is perfectly liquid, scarcely different from serum, and perhaps, like serum, is but little else than dissolved albumen. In more advanced cases, the tissue of the lung, in which organ the tuberculous deposit is most common, passes through several different changes. It either becomes of a dull opaque, white, and quite hard, or it is of a uniform yellow colour. When you examine a portion in which the yellow colour is well marked, you will probably regard it, at first sight, as a perfectly uniform homogeneous mass; when, however, you examine it more minutely, especially if aided by a weak magnifier, you will find that it is divided into an immense number of minute granulations, closely adherent together, forming, in fact, a single mass. It would seem, therefore, that the tuberculous matter, in becoming solid, always assumes a rounded or granular form. In other words, this is one of the distinguishing characters of tubercle, just as the striated arrangement of its particles characterizes scirrhus.

It is extremely probable that the most minute granulations are contained in a distinct cellular cyst; that is, that the blood-vessels nourishing them are contained in a separate cellular membrane, separating it from the rest of the tissue. This membrane is perfectly demonstrable when the tubercle is somewhat advanced in size; while it is still a mere granulation, we can only infer its existence from the general analogy of morbid structure. The cyst does not exist in a separate form when the tubercle is formed upon the free surface of a secretory membrane, as in the granulations which I have already described, where the peduncles of the tubercle evidently extend along the smaller bronchial tubes. In these cases, the nutritious envelope is the mucous membrane itself. When the tubercle grows in size, so as to distend the bronchial tube, the mucous coat begins to ulcerate, and a cellular cyst immediately forms.

I have no doubt that the fluids necessary to the growth of tubercles, pass from the circumference towards the centre. At the same time, there is no process analogous to a true circulation, but there is a sort of inhibition of nutritive fluid, which permeates the tuberculous matter. The reasons which sustain this opinion are numerous. Tubercle is evidently not a perfectly inert mass; it is in a very different condition in the tissues, and after its removal from the body. If gangrene should supervene, the tubercle quickly becomes softened, and falls into a

grayish, fœtid pulp, different from the thick fluid which is formed during its natural or regular process of softening. Tuberculous matter remains permanently moist, or at least it becomes dry only when it ceases to grow, and is about to disappear by absorption of its more liquid parts. Hence I infer that its inferior degree of vitality is like that of cartilage, or of the other tissues of the body, which have neither nervous nor visible vessels. Indeed, we may extend the analogy still further; for the tissues of the body which are apparently the best furnished with blood-vessels, are in reality merely interspersed with capillaries, which are numerous enough to give them a general red colour, but do not properly enter into the tissue of the organs. Tubercle and other morbid growths differ, however, from the normal tissues in one very essential particular; they have no proper function in the economy—they are the mere result of a diseased action, and, after their formation, they tend naturally to destruction and subsequent diminution.

Tubercle does not always terminate in the same manner. The most frequent mode is by a process, which is termed softening. This takes place after the tubercle has attained a certain size, and begins in several different ways. One is evidently from the circumference towards the centre. The tubercle is gradually surrounded by a thick fluid of a yellow colour, which possesses the characteristic properties of pus; this gradually becomes thinner, and the tuberculous matter composing the mass, is blended with it. The tubercle itself is gradually resolved into a homogeneous fluid; in part, it seems to be evacuated in substance, in the form of irregular grains. Sometimes these portions of half solid tubercle are of considerable size, and include fragments as large as an ordinary pea, and in a few cases they are even much larger. The tuberculous matter forms in most cases so perfectly homogeneous a liquid, that there is every reason to believe that it is really dissolved in the purulent secretion. The resulting liquid is, however, thicker than water, and of a dirty yellow colour; it may be examined by you either in the recently formed cavities of the dead subject, or in the sputa. When the softening has taken place with great rapidity, the sputa consist entirely of this glutinous liquid, which collects together in the cup, and assumes very nearly the appearance of thin bookbinders' paste. It was stated by Laennec, that tubercle always softens from the centre towards the circumference. This, however, is far from being the case; on the contrary, in the majority of instances, it is as I have just stated, from the circumference towards the centre. But, when we examine tubercles of a certain size, we often find that their centre is already softened, and the central parts remain consistent. This is best seen in the large tubercles of lymphatic glands, in which we often find a softened pulpy mass occupying the centre of the gland, while the circumference is still hard. It is difficult to explain the process of softening in these cases, without supposing a sort of imperfect cir-



culatation in the tubercle, which conveys to the centre fluids in sufficient abundance to break down the tissue, and reduce it to a pulp. I am disposed, as I have already stated, to admit that there is a sufficient transmission of fluids through the tubercle for purposes of nutrition; but the mode in which softening of tubercle often takes place, seems to prove that the supply of fluid may be much increased, and that it is perhaps changed in quality, so as to break down the substance of the morbid deposit. Both these modes of softening have been, by turns, admitted and denied; but there is no reason for adopting either of them, as an exclusive opinion;—on the contrary, we have seen that they both occur in different individuals, and under various circumstances.

There is a third mode of softening, or at least of destruction of tuberculous matter; that is, gangrene. This sometimes takes place in the advanced stages of phthisis, when the strength of the patient begins to decline, particularly if a tendency to gangrene should characterize the prevailing diseases. The tuberculous mass loses its yellow colour, and assumes a greenish tinge, at the same time it is bathed in a fœtid liquid, which is in part secreted by the neighbouring tissues, and in part arises from the mortified tubercle. The decomposition of the tubercle goes on almost as rapidly as the same tissue, if removed from the body, and exposed to moisture. This is an additional proof of the semi-vitality possessed by the tuberculous matter. Gangrene sometimes attacks the tissue around the tubercle, but in most cases it is limited to the morbid product itself.

You have already seen a sufficient number of cases of tuberculous disease to convince you that in all cases in which this matter is softened, it has a natural tendency to the exterior; that is, it obeys the same laws as pus. The passage to the exterior can only take place by means of ulceration, which always proceeds in the direction of the nearest outlet to the exterior. Hence the mucous membranes which pass near, or into those parts of the organs where the deposits of tubercle are most abundant, afford a ready passage to the softened matter. When the tuberculous matter is in contact with the serous membranes, the process of softening is more slow; indeed, it seems as if it were wisely retarded, in order to prevent the consequent perforation and inflammation of these tissues.

I have already stated to you that tubercle is generally discharged under the form of a homogeneous liquid, although you may sometimes detect portions of solid tubercle in the sputa; the softened tubercle cannot be readily studied in other organs than the lungs, as it is either mixed with the proper excretions of the part, and connected by them, or it softens but slowly. In whatever part of the body the softening occurs, you always detect the distinct membrane, or rather two membranes which surround the soft pulp. Of these the exterior is hard and semi-cartilaginous, and seems to be formed by the ori-

ginal shell or investment of the tubercle; the internal is comparatively soft, and of a dull yellow colour. The latter membrane secretes pus, and differs in no respect from that lining an ordinary abscess. This internal membrane is necessary to the formation of granulations, and cicatrization of the cavity.

Cavities following the softening of tubercle, certainly tend towards cicatrization; that this is their natural course, is established by a variety of evidence. Unfortunately, however, many circumstances often occur to interrupt it; of these the most frequent is the simultaneous development of tubercle in other organs, or in other portions of the same organ. Hence the irritative fever reacts upon the whole economy, including the walls of the cavity, which continue to secrete pus, instead of gradually contracting by the process of granulation. Cicatrization of these cavities, however, is not unfrequently completed, and sometimes the patient remains well for a long period after the elimination of the tubercles, especially if he is not the subject of a strongly developed constitutional tendency to the disease.

In proportion as cicatrization advances, the soft inner membrane gradually becomes less apparent, and the outer one increases in hardness. At last it forms the entire wall of the cavity, or rather the inner membrane has formed a close adhesion with it, and has gradually ceased to be a pus-secreting membrane, assuming more and more the characters of a mucous surface. I say a mucous surface, because this discharge of tubercle, and consequent cicatrization, never occurs except in the mucous membranes, such as those of the lungs or the intestinal canal, or in the skin, or in cases of tuberculous disease of the lymphatic glands. The opening from the cavity towards the exterior usually remains patulous, becoming a real fistula, and sometimes it continues to secrete a little puriform fluid. In many cases the orifice has become so large, that the cavity is altogether continuous with the bronchial tubes, or the intestinal canal, and is, therefore, hardly perceptible, without a careful examination, and you will find no other trace of the healed tubercle than a sudden termination of the bronchial tube in a cul de sac, the part beyond the tuberculous deposit being obliterated. The contraction of the tissues is not confined to the mucous membranes, but, as you may readily believe, when you reflect on the analogy of these cavities with external abscess, the adjacent tissue is puckered and wrinkled; this gives rise to a very singular appearance both in the lungs and intestinal canal.

The softening and evacuation of tubercle is not the only means offered by nature for the cure of this disease,—in some cases the deposit becomes dry, and is gradually converted into a calcareous mass. The process is analogous to the change in some of the fibro-cartilaginous tissues, which occurs in advanced age. The calcareous portion is really increased in quantity, while the more highly animalized part is really diminished in proportion. The cyst becomes stronger and



firmer, and closely embraces the calcareous deposit, but in very old cases is at last absorbed, so that nothing but a portion of osseous substance remains. The connection between a bony deposit and an ordinary tubercle, is not very evident when we examine them at very different periods of the disease; but if the progress of the tubercle be observed throughout its course, you will find that the transformation is extremely gradual. At first the tuberculous matter becomes nearly dry; it soon assumes the consistence, and very nearly the appearance of half-dried plaster. As the calcareous substance is more abundant, it becomes rough to the touch, and finally consists either of a single mass of bone, or of a gritty substance which scarcely adheres together.

The calcareous tubercles are supposed by some to be of a different kind from the ordinary tuberculous deposit. This is certainly not the case. I have watched the progress of the change in the bronchial glands of children, where the lesion is most easily studied, and could observe nothing peculiar, except that the disease was slower in its progress, and therefore more apt to be arrested. If the tuberculous affection be of the acute kind, it is very certain that the only termination of tubercle will be softening, or death before this stage is reached; but if the tubercles be large, and the further progress of those already formed, as well as the secretion of new tuberculous matter, be arrested, the disease will terminate by the absorption of the less solid parts, and the deposit of the calcareous matter. The calcareous matter is scarcely found except in the lungs and the lymphatic glands, perhaps because the tubercles rarely attain a large size, except in those organs. Many physicians are inclined to deny the identity of the calcareous substance and of ordinary tubercle, because they have associated in their minds the ideas of death and of tuberculous disease so firmly, that they are unable to conceive of the deposit of tubercle without a fatal termination.

The last point to be ascertained is, whether tubercles are ever absorbed, without either softening or the formation of calcareous matter. This is a very difficult question; for, from its nature, it does not admit of exact demonstration. I am quite convinced, however, that such is the case. My reasons for this belief are, that many patients present decided signs of the occurrence of tuberculous disease, who recover completely from its effects. In a few of these cases I have had opportunities of verifying the completeness of the recovery. After the accidental death of the patient, from some acute disease, unconnected with the tuberculous affection, I have then ascertained that there was no trace of tuberculous matter,—while in another set of patients, the disease went through its stages regularly, and ended fatally. The weak point in the chain of evidence is, of course, the doubt that may remain upon the minds of many as to the certainty of the diagnosis in those cases which did not end in a decided tuberculous affection. It is a question of probable evidence, not of rigorous demonstration. For

me the proof is conclusive, but I have no means of bringing others to the same opinion. I do not believe that, under any circumstances, the evidence can be much more complete;—doubt must always remain, especially in the minds of those who think that a tuberculous deposit is unavoidably the precursor of death. This is, however, not my own opinion; and I can state my entire conviction, that some cases in which tubercles were actually formed, end in absolute recovery—and I am also able to state that this conviction is derived from no small share of observation.

## CLINICAL REPORTS.

### PHILADELPHIA HOSPITAL.

*List of Accidents admitted into the Pennsylvania Hospital, from July 1st to July 30th, 1839.*

[Reported by HENRY WHEATON RIVERS, M.D., Resident Surgeon.]

A case of compound fracture of the skull, caused by a bolt having a head three quarters of an inch in diameter, being driven through the frontal bone; it was forced in so firmly that his fellow-workmen were unable to extract it until surgical aid was called in; it is now five weeks since the accident and the patient is doing well.\* A case of contusion, accompanied with some concussion of the spinal marrow and brain, caused by a fall from the fifth story of a house, treated by cups along the course of the spine and cold applications to head; removed from the house by his friends the day after the accident. A compound and comminuted fracture of the left foot and ankle, caused by a rail-road car passing over it; this case was admitted on the evening of the 4th of July, the leg was amputated immediately, the anterior tibial artery was found to be ossified, it was secured, however, by a ligature which has since come away; the patient, æt. sixty years, is doing well. A case of contusion of the hip, by a blow from a large log of wood, treated by the application of six wet cups to the part, and keeping at rest. A case of comminuted fracture of the left clavicle, accompanied with contusion of the right hip, caused by the passage of a loaded cart obliquely over the body; died in four days of mania a potu. A lacerated wound of the scalp, dressed with adhesive strips and cold wet cloths; died on the fifth day after his entrance of mania a potu. A case of lacerated wound of the scalp, caused by falling down a flight of stairs in an epileptic fit; the laceration was so extensive in this case that it was found necessary to bring the parts together with sutures, the patient had no bad symptoms and was discharged, cured twenty-four days after the accident. A case of lacerated wound of the scalp, including the right ear, which was partially torn off, produced by falling from a dray, the wheel of which passed over the side of the head. The treatment consisted in replacing the ear by means of sutures, a compress in the meatus, and a tight bandage round the head; the

\* A full report of this case, which is a very interesting one, will be furnished at its termination.



wound is now healed. A case of punctured wound of the leg, from a stab with a large knife. A case of incised wound of the arm; died in four days of mania a potu. A case of compound and comminuted fracture of the left foot and ankle, from the passage of a rail-road car over it; this was a boy *æt.* five years; the leg was amputated by Dr. NORRIS, on the day of his entrance, (July 12th,) and the stump is doing well. A case of contusion of the ankle, caused by a blow from a ball in a ten-pin alley; treated by leeches, afterwards by soap-plaster, and keeping the leg at rest in a fracture-box; discharged cured in eleven days after the accident. A case of rupture of the ligament of the patella, produced by a fall on the knee while the leg was in a state of flexion; the treatment in this case was the same as would have been pursued in a fracture of the patella, viz., by elevating the limb on an inclined plane, a long splint on the under part of the leg, roller and compress. A case of contusion of the shoulder, caused by a blow upon the part, treated by cups, and rest; discharged cured thirteen days after the accident. A case of fracture of the clavicle near its outer end, treated by Dr. Fox's modification of Desault's apparatus. A case of contusion of the ankle, caused by being run over by a dray, treated by leeches, cold applications, and keeping the leg at rest in a fracture-box. A case of lacerated wound of the hand, from the bursting of a gun, treated by keeping the hand and arm at rest in a box of bran, with a view, if possible, of saving the hand; this patient was taken home by his friends two days after the accident. A case of contusion of the ankle, treated by soap-plaster, and being kept at rest in a fracture-box. A case of burn of both arms, caused by boiling water being spilt on them, dressed at first with olive oil and lime water, and afterwards with Goulard's cerate; doing well. A case of fracture of the ulna, about its middle, caused by a blow directly on the part, dressed in the usual way. A case of secondary hæmorrhage from a wound received three weeks before in the palm of the hand; a week before his entrance, he had hæmorrhage which was suppressed by a ligature round the radial artery, by Dr. G. M'Clellan; in this instance he applied to Dr. M'Clellan, who was absent from town, and on that account was sent to the hospital for the purpose of having the ulnar artery secured also, but there being no hæmorrhage when he arrived at the house, a compress was applied with a splint and tight bandage, cold applications were made use of, and the limb elevated on an inclined plane of pillows, since when, he has had no return of the hæmorrhage, and the wound is nearly healed. A case of wound of the hand caused by the discharge of a pistol, the wadding of which entered the palm of the hand, and was taken out five days after his entrance, (a poultice having been applied,) from the back of the hand near the thumb: a great deal of suppuration has taken place; the treatment has consisted in keeping the hand elevated and at rest; poultice to hand, and cold applications to arm. A case of contusion of the

wrist from a blow directly applied, treated by keeping the arm and hand at rest in splints, and cold applications to wrist. A case of fracture of the neck of the thigh bone, in a female, *æt.* eighty-six years, treated by keeping the limb at rest on a double inclined plane of pillows. A case of compound and comminuted fracture of the leg, from the passage of a rail-road car over it; reaction did not take place until about two hours after his entrance; the leg was amputated on the following morning by Dr. NORRIS, and the stump is now doing well.

## THE MEDICAL EXAMINER.

PHILADELPHIA, AUGUST 10, 1839.

ONE of our correspondents furnishes us with an analysis of the debates of the Royal Academy of Medicine, of Paris, on the subject of the distinction between the nerves of motion and of sensation. These debates are almost interminable, and occupy a large portion of the late numbers of the Bulletin of the Academy of Medicine. Like the debates of most learned societies, they have ended by leaving every one precisely of the same opinion which he held at the time they began;—we ought not, on that account, to conclude that they have been altogether fruitless. Arguments in medicine, as in every thing else, do not convince, but they often awaken reflection, and give rise to new and more exact observations, and thus indirectly elicit the truth.

The composition of the Royal Academy is like that of most other scientific societies,—it consists of men who have attained distinction by glorious and successful contributions to science, and of others who are admitted to a seat on the strength of a doubtful and almost surreptitious reputation. Little is positively done by such bodies;—those who are truly capable, have generally reached that period of life when repose brings with it more attractions than labour, and the mere charlatans in science are as useless occupants of the places of the Academy, as of the chairs of the School of Medicine.

ERRATUM IN No. 28.—In Dr. Wooten's paper on Dysentery, page 438, line 35, for "*Pulv. ipecac. et opii aa gr. xx.*," read *Pulv. ipecac. et opii gr. x. ad xx.*

## DOMESTIC SUMMARY.

THE milk sickness is a disease, of which no satisfactory account has been published. It is limited to certain portions of the United States,



especially Kentucky, Ohio, Indiana, and Illinois. Indeed, we believe it is not only confined to those states, but to a very small portion of them. It occurs in a particular and often limited locality, and, after a few years, ceases. Cultivation seems to arrest it; whether this is always the case, is unknown.

The disease is supposed to be caused by poisonous vegetables eaten by cattle. When communicated to the human subject, the mouth or flesh of these animals produces the symptoms of most acrid poisons, and most evidently causes a local inflammation, with a depressing action upon the nervous system.

The following article is very incomplete; nevertheless, as the writer seems to have seen much of the disease, it may be interesting to our readers. We would invite our correspondents who reside in the districts of country where the disease prevails, to furnish us with a more satisfactory account of it. It would be interesting to the profession, both in the United States and in Europe.

*Extract from an Inaugural Thesis on Milk Sickness.* By DAVID L. SIMPSON, M.D., of Owen County, Kentucky.—I have been raised in a section of country, where this disease has prevailed from its earliest settlement.

The general face of the country is broken, but the soil is rich, producing lofty timber and a luxuriant crop of native vegetation, upon which cattle graze and fatten, and large numbers range, indiscriminately, upon hills and dales in the forests, and often escape the disease.

The cause of the disease is involved in obscurity, and it is only by analogy that we can arrive at any certain conclusion upon the subject; but, that a disease (*sui generis*) exists, or is produced in certain districts, which, out of those districts, was never known, is a fact, which the most sceptical can no longer deny.

I have often heard it observed, by the oldest settlers of the country, that excessively dry seasons were most productive of the disease, and my own observation goes to establish the position.

The spring and fall of the past year were both uncommonly dry, and I have never known a season so fatal to stock.

The following is a summary of facts, connected with the history of the disease.

1st. That it is confined to certain limits, and that the formation of the surface, soil, timber, &c., in those districts, corresponds with that of adjoining districts, where the disease was never known to prevail.

2d. That the milk of the cows produces the disease in calves, and persons that partake of it.

3d. That cows giving milk that is poisonous, are seldom affected by the disease themselves, while dry cattle that graze with them will sicken and die.

4th. That the disease is communicable to man through the medium of beef.

5th. That some districts, which in their native state were fatal to stock, but which are now in cultivation, are free from the disease.

It has been customary with the pioneers of the country, to belt or girdle their timber, several years before they intend to clear the land for cultivation, and the surface of such land is soon covered with rank vegetation. Many observers suppose that these *deadening*s (as they are called) are more dangerous to stock than the native forests; whence it has been inferred, that the disease is of vegetable origin, and that the noxious plant is eaten by our stock, only when other vegetation is generally scarce.

I have read a paper, written by G. W. Wright, of Cincinnati, Ohio, in which he advances the doctrine, that the disease is of *miasmatic* origin; but the fact of the locality of the disease, and that locality corresponding so nearly with adjoining healthy districts, is sufficient, without argument, to confute his theory.

He has advanced another idea, which is dangerously erroneous, viz.: that the disease cannot be communicated by beef, because a case was never known in Cincinnati, where beeves are constantly slaughtered that are driven from Kentucky, where the disease exists.

It is a fact well known to all acquainted with the disease in cattle, that those actually sick cannot be driven many miles, without exhibiting symptoms of disease; and if they are hurried, they will tremble, sicken, and die. Hence the great caution taken by the owners of stock, when they find them sick in the woodlands, to drive them very gently to their farms.

Many experiments have been made (with the milk of suspected cows) upon dogs, and the disease has been produced upon them in a few days. When cows die of the disease, great care is taken to keep valuable dogs from eating the flesh, because of its pernicious quality.

It is my opinion, that the cause may exist and lie dormant in the system, for a long time, and finally pass off without harm, unless it is developed by some exciting agent.

The prominent exciting causes of the disease in men, are over exercise, and excess in drinking *spirituous liquors*.

The symptoms of the disease, in its first stage, are, more or less languor, indisposition to move, and fatigue upon the slightest exertion.

The digestive functions are but slightly impaired, the appetite good, the tongue natural, and all the secretions seem undisturbed; but if the patient, in this condition, exercises so much as greatly to excite the circulation, or drinks freely of any stimulating liquor, that will have the same effect, the disease will be developed in its worst form, and be ushered in by nausea and vomiting of acrid bilious matter, a sensation of burning in the stomach, excessive thirst, obstinate constipation of the bowels, great depression of spirits, coldness of the extremities, offensive breath, a dull expression of the eye, great restlessness, a



violent throbbing of the arteries of the abdomen, while the circulation in the extremities is below the natural standard, breathing laborious, coma, and at last death.

I have, though rarely, found my patients chilly, and when that symptom occurs, a slight febrile state of system follows, as a matter of course. Yet both are accidental attendants upon the disease. No pain is complained of, and the only cry of the patient is for cold water, ice, &c. &c. The sickness attendant on this disease is peculiar, and I do not recollect to have seen a patient, (that had all the symptoms fully developed) who was not under the impression that he would die of the attack. The pulse, in the early stage, is full and often slower than natural; but as the disease advances, it becomes quicker and smaller, and finally indistinct at the wrist, while the action of the heart and large arteries is violent. If I were to name a disease, to exemplify the term *congestive*, it would be this; as no disease that I have ever seen, is so completely of that character.

When I have found my patients with cold feet and hands, I have frequently asked them if they were not uncomfortably cool, and the reply has invariably been in the negative. If re-action ever ensues, I consider the patient out of danger.

The *treatment* of the disease is simple, and such as would naturally be suggested to an experienced physician.

I usually give alkalies, to correct the acidity of the stomach, and these are followed with cathartic medicine. Calomel, on account of its specific gravity, is preferable to any other cathartic, in the early stage of the disease. I have given it, in large doses, until seventy-five or one hundred grains were retained by the stomach, and afterwards, castor oil, followed with aloes, rhubarb, senna, and the like.

The drastic vegetable cathartics are seldom retained upon the stomach, though when the vomiting ceases before the bowels are acted upon, I often use them beneficially.

The motto of my preceptor is, "never to give up the ship," as success often attends persevering efforts, even after the hope of the patient and his friends is gone.

Early attention to the extremities is important. Frictions, with warm stimulating applications to the epigastrium and extremities, should never be neglected. I have often resorted to blisters over the stomach, but am not satisfied that they have any claim to superiority over sinapisms and other rubefacients.

Heated spirits of turpentine, applied to the surface, and covered with coarse brown paper, is a valuable remedy to restore the circulation. Enemas should never be neglected, and should be given every two or three hours, until the regular action of the bowels is restored.

A saturated solution of Glauber's salts is often used with success; but I have never confined myself to the use of any specific remedy. When one fails I try another, till I conquer the disease, or till the disease conquers my patient.

I have in a few cases tried croton oil, but cannot say much in favour of its use.

The great and only difficulty is to overcome the torpor of the bowels; and when that is accomplished, I feel satisfied. It is necessary to attend strictly to keeping the bowels soluble during convalescence, which can easily be effected with castor oil, or any simple cathartic. Flowers of sulphur, combined with cream of tartar, constitute a favourite remedy with me in this stage of the disease. The disease has often been treated successfully, by the early use of tartar emetic, and when it acts as a cathartic, is perhaps the best medicine. When it fails in this, the powers of the system often seem exhausted by the use of it, and hence I cannot regard it as a safe remedy.

Bleeding has been much urged by theorists,—but I have never tried it, and am induced to believe that it is unsafe, from the character of the disease, and from the effect that it is said to produce by those who have tried it.

My preceptor informed me, that in every case, when he tried the lancet, the patient seemed to sink immediately, and that he has never known a case of recovery, where bleeding was resorted to. On the other hand, I have found active stimulants beneficial, after the operation of purgatives, and frequently have given brandy, to relieve great prostration, even before medicine had acted upon the bowels.

I have now presented a simple detail of facts, as regards the history of the disease, its symptoms and treatment; and, for fear of wading into too deep water, I will try to get out, without venturing further into the intricacies of the subject.

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## FOREIGN SUMMARY.

*Clinical Observations on Fever.* By CHARLES LENDRICK, M. D. T. C. D., Queen's Professor of the Practice of Medicine in the School of Physic in Ireland.—Fever was described in the synopsis of my course of lectures on the *Practice of Medicine*, published in the year 1833, as a disease dependent on "*morbid actions not referrible to any particular structure or system.*" In thus denying the essential connexion of fever with organic lesions, or morbid changes of the fluids, I did not, nor do I now, claim originality. In my opinion, the priority as to time, in alleging a fact, is but a poor test of professional knowledge; and the palm ought to be assigned, not to him who (perhaps accidentally) stumbled on the truth in the first instance, but to those who, in opposition to fashionable theories and received doctrines, steadily upheld what proved to be established on the most solid grounds. In making this remark, I wish to be understood as referring to the expressed opinions of able and experienced practitioners in Dublin, who, for probably half a century, have, through evil report and good report, denied the hypotheses of both solidists and fluidists, as explaining the source of fever.

I am not an advocate for *authority* in medicine;



yet I confess that when, many years ago, I considered the extensive opportunities that Dublin affords to the practitioner, in fever—the enormous prevalence of that disease in our metropolis—the excellence of its principal institution (in Cork street) for the reception of fever patients—and the consummate skill and experience of such men as Quin, Purcell, Harvey, Plunkett, Perceval, &c., I felt, even at a very early period of my career as a medical man, an inclination to doubt the theories so imposingly put forward by authors whose opportunities were so much inferior to those of the distinguished practitioners I have just mentioned, and which, in my opinion, were the less deserving of confidence, from the overweening confidence with which they are urged.

On perusing, several years since, Travers' work on Constitutional Irritation, I was struck by the coincidence between his statements and my own experience. I asked myself the question repeatedly—"Did I not know all this before?" It seemed to me that I had learned nothing new, as is the case with most persons *after* they have become acquainted with the discoveries of others. Medical discovery, however, generally merely renders clear what was formerly obscure; and we are too apt to confound the twilight of our own recollection with the clear elucidation of the subject by others.

When I had methodized my thoughts, after a perusal of Travers' work, and its comparison with (or rather confirmation by) what I had observed, it seemed to me that there was no important distinction (contagion excepted) between fever and what he termed *constitutional irritation*. A fracture—a contusion—a wound—any injury, however slight, appeared to be capable of producing all the phenomena of *fever*. Delirium—coma—convulsions—a surface, hot, cold, or flooded by perspiration—excessive and depraved secretions, or a stoppage of these secretions—every imaginable variation in the state of the human economy was capable of being produced by the most trifling irritation of the *nervous system*, under peculiar circumstances.

Here is an important point established in our analysis. A similarity of effect goes a certain way in proving a similarity of cause. The identity, however, of fever with constitutional irritation admits of further proof.

1st. In both diseases the influence of an *external* cause is requisite to produce the effect. In that of constitutional irritation, the impression of a local irritant on the nervous system is obvious, from the effect of the external injury. In the case of fever, the nature of the influence of a cause external to the system is much more obscure; but the necessity of such a cause in order to produce the usual effect is undeniable. What are called the "accessory" causes of fever—cold, mental anxiety, intemperance, &c. are quite inadequate *in themselves* to its production; they may establish an inflammatory or nervous disease, but they will not cause *typhus fever*, unless typhus be prevalent in the district at the time, or unless the patient has been exposed to contagion. Then, the means

which on other occasions produce other diseases will engender typhus fever of the worst description. In *contagion*, and what are called *epidemic* and *endemic* agency, whether combined or uncombined with the *accessory* causes of fever, we therefore, have a principle, which in this disease is equivalent to the influence of an external injury in producing constitutional irritation.

2dly. In both fever and constitutional irritation, we observe the same influence of the constitutional powers of the individual affected. While all the symptoms of this morbid state are produced by an external injury, a similar, or a much greater injury, will be borne by another patient with not the tenth part of the constitutional disturbance. So in the case of fever, several persons will be similarly situated as to the influence of pestilential agency or contagion; yet one will be attacked by typhus fever of intense severity, another will labour under merely a *febricula*, while a third will escape altogether.

3dly. Organic changes take place in both diseases under similar circumstances. If the patient be predisposed to any organic affection, it is sure to display itself during the perturbation of the system consequent on the establishment of fever or constitutional irritation. This will happen even at an early period, and to a considerable amount, if the patient has been much exposed to the operation of those agents, cold, vicissitudes, &c., which are accessory in the production of the febrile affection, and which exert the principal influence in forming organic disease.

Constitutional irritation thus renders us powerful pathological assistance, by showing the capability of an external and remote irritation of the nervous system to produce *organic disease*, as well as a constitutional disturbance similar to fever. A man receives a bruise on his head; severe constitutional disturbance supervenes; he displays symptoms of local disease in the thoracic or abdominal viscera; he dies, and the expected organic changes are discovered. Here no one can say that pneumonia or hepatitis was the *cause* of the disease, which is distinctly referrible to an obvious injury inflicted on a limb. Hence, we arrive at the conclusion that the *occasional* appearance of certain organic changes in *fever* ought not to induce us to refer the cause of this disease to them. We cannot trace out its exciting cause, contagion, &c., with such facility as in the case of external injury; but the circumstance that there is an external cause in operation, (although more securely,) and with nearly similar results, in the one case as in the other, and that in both there are the same variations as to the occurrence of organic affections, whether as to this taking place at all, or in any particular form, afford confirmation to the great similarity (or nearly identity) of fever and constitutional irritation. Now as the latter is clearly referrible to an external impression made on the nervous system, modified by the constitutional idiosyncrasy of the individual, and by the operation of what are termed accessory causes, ought not FEVER to be referred



to a similar source—with the substitution of contagion, or epidemic, or endemic agency, for injury, &c. ?

Some say, "Any pathology rather than none." This I deny. The person who knows by experience merely that a certain remedy applied in a certain way will cure a disease, and who proceeds on this principle, is a far safer practitioner than he who adopts erroneous opinions, and *acts* on them. The reason that false pathology has not done so much mischief as might have been expected is, that practitioners often theorise about disease in one way, and practise in another. Their theory is unfounded, but their practice, based on their experience, and not on their avowed principles, is correct.

Among the many physicians who have been distinguished in Dublin for their skill in the treatment of fever, none perhaps have attained to so great a celebrity as the late Dr. Harvey, nor has there appeared since any one so conspicuous for the successful management of what seemed to be desperate cases. I endeavoured, at an early period of my professional career, to make myself acquainted with his rules of practice; and the most accurate information I could acquire was, that his great secret consisted in doing "*very little*." It was a sarcastic remark of his, that "the patient was in greater danger from the doctor than from the disease."

It is not unlikely that a modification of this remark of Dr. Harvey is applicable to many diseases besides fever. It is still in reserve for us to learn, how disease will subside without the daily administration of medicine, or how far it is a provision of nature that it shall run its course, and then terminate favourably. For instance, a principle most confidently inculcated formerly, that the tendency of cholera is always to a fatal termination, and that the patient is never saved but by art, was disproved by the fact frequently observed in Ireland, that paupers left in the last stage of cholera asphyxia to die in a ditch, recovered notwithstanding, and without the use of any medicine but cold water.

"What is to be *done*," is a frequent question at medical consultations. It is often, however, of more consequence to know what is *not* to be done; and the practitioner often cannot display greater skill in fever than by knowing when to do nothing. By adopting the negative mode of treatment in the great proportion of cases, and by interfering in the others, only to the amount actually indispensable, during my attendance at Sir Patrick Dunn's Hospital, when fever was unusually prevalent two years ago, I lost but two patients; one of whom was a confirmed drunkard, and the other laboured under long-established emphysema of the lungs, complicated with bronchitis. For the latter disease, rather than fever, she was admitted into the hospital.

It is fully established, that patients will recover in a great majority of instances, from typhus fever, by the almost unassisted powers of nature. In many such cases, especially among the lower orders of society, such is the torpor of the func-

tions, partly from the disease, and partly from previous habits, that a medical attendant can scarcely do harm by any practice confined within reasonable bounds; and whether he bleeds and purges on the one hand, or uses bark and wine on the other, provided he does so moderately, he will not *prevent* the patient's recovery. This fact has not only led to the undeserved reputation of alleged specific modes of treatment in fever, by a coincidence of spontaneous recovery with their supposed effects, but has also caused an opprobrium to our profession, namely, that statistical returns prove that the average amount of mortality is nearly the same under one mode of treatment as another. The reason of this, however, is, that the *average* mortality is influenced by the *large* proportion of cases, which are of the description I have mentioned, where almost any treatment or no treatment is equally successful as to the result. It is in the *small* proportion that medical skill and the effects of medical treatment are shown; and these, from their fewness, but little affect statistical returns. In such cases the practitioner is constantly between Scylla and Charybdis; and whether the necessities of the case require bleeding, purging, mercury, wine, or other stimulants, whatever is done beyond what is actually *necessary*, is done injuriously.

Typhus fever presents itself in two forms; typhus, properly so called, and the synochus of Cullen, where local inflammation to a greater or lesser extent is combined with typhus. The distinction of such cases, and the mode of treatment, will be referred to hereafter; for the present, I cannot too strongly urge the danger of being led astray by that perturbation of the system which once led to the hypothesis of a fermentation of the fluids; and under the operation of which, local disease, which in itself might require active and special treatment, is liable to be simulated. Thus, with disturbance of local functions, or determination of blood to various parts, unusual secretions take place in the respiratory organs, as elsewhere; and which, on the application of auscultation, seem to depend on organic disease. I have been repeatedly assured by others, after the application of the stethoscope, that the lungs were softening, or that latent pneumonia was present; and yet the physical signs subsided, without any direct treatment, on convalescence from fever being established.

I particularly distrust *crepitus*, when existing merely in the *gravitating* portion of the lung. I have repeatedly observed it at the back of the thorax, when I am convinced from the progress of the case, that it depended merely on a general secretion which had become accumulated there in consequence of the supine posture of the patient. I therefore rarely direct any special treatment for *mere* crepitus, when occurring in typhus fever; inasmuch as there is less danger in allowing pneumonia, if present, to further develop itself, than to venture on bleeding, mercurialization, or the action of the tartarized antimony, *unnecessarily*, in typhus.

[To be continued.]